of motion that a body, if moving, will continue to move in a straight line with uniform velocity, provided no forces act on it. There is through the whole book the most hopeless confusion as to what is meant by no forces acting. The author's theory of the cause of the moon's motion will illustrate the character of the book. It is briefly this:—If the moon were pulled only by the earth's gravitation, it must fall to the earth. It must therefore be pulled in the opposite direction with an exactly equal force. This is called by the author the force of astral gravitation. Since these two exactly balance one another, the question arises, What can cause the moon to revolve round the earth? "A cause," says the author, "for the onward motion of the moon according to the action of well-known laws, is, however, indicated by various well-known tidal phenomena. For the moon tends to raise a mass of water or tide on the earth's surface beneath it; and as the earth's surface rotates eastward, it tends to carry that mass of water or tide with it; and therefore as the moon tends to hold the tide beneath it, the rotation of the earth eastwards must just as certainly tend to carry the moon eastwards as to carry the tide eastwards." Really it is quite irritating that such nonsense should be printed in large type, on good paper, and in a well-bound book.

It is one of the great merits of error that it is inconsistent. That the author enjoys the full satisfaction of this merit is evident from the following:—"If the moon and earth were at rest, then astral gravitation would tend to carry the moon directly from the earth, not at a tangent to any part of its orbit; and when in motion, astral gravitation tends to carry the moon off at a tangent from its orbit certainly, but *in the opposite direction to that of its motion* at the moment of its being released from the grasp of the centripetal force." The italics are the author's—not ours. It appears from this that the author denies the second law of motion, as well as the first, which he elsewhere denies. He here assumes that the effect of a force on a body at rest can be at right angles to its effect on that body when in motion. It is much to be desired that those who undertake to write books would first learn the first principles of that which they write about.

Catalogo Poliglotto delle Piante. Compilato dalla Contessa di San Giorgio. 8vo., pp. i., 747. (Firenze, 1870.)

WE wish to call our readers' attention to this interesting little volume. Its authoress will be better known in the country as Lady Harly of Oxford; and she has spent many years in the compilation of this volume, which, we think, may prove useful to travellers on the Continent, and indeed, might even be made the means of instruction in our public schools. As an example, we select the *Bellis perennis*, which, a native of Europe, we find is, in English Daisy; French, Paquerette, *Marguerite vivace*, Fleur de Pâques; Italian, Pratolina, Margheritina de Prati; Spanish, Maya, la Margarita; German, Masslieben, Gänse Blümchen, Angerblume, Osterblümchen. But we find not only the European names of a large number of plants given, but, even in some instances, the Sanskrit, Bengal, Hind and Tamul names are also appended.

W.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Variability and Natural Selection

I ASK your permission to address to your readers some observations in support of the argument which Mr. St. George Mivart has adduced in his work on "The Genesis of Species," that Mr. Darwin has attributed too exclusive an effect in the origination of new species to the influence of his law of Natural Selection.

1. There are two facts on which Mr. Darwin's theory reposes—the one the variability of animal and vegetable forms; the other, the accumulation of a useful variation by force of the struggle for existence, or Natural Selection. Mr. Darwin assumes Variability to be accidental, not because he or any other philosopher supposes that there is such a thing as accident, but because its law is unknown. He then finds the law of Natural Selection, and by this and this alone, explains the Genesis of Species.

Now, inasmuch as Natural Selection operates on Variability as a pre-existing fact, it follows that the genesis of a new species must result from the operation of two laws—namely, the law of Variability and the law of Natural Selection. But Mr. Darwin attributes it exclusively to the latter; so that, according to his view, there is a law (that of variability) which has no part in producing a result which yet depends for its existence on the fact regulated by that law—namely, variation. This is enormously improbable. It is as if the resultant from two factors were attributed exclusively to one of these factors.

The fact that Mr. Darwin assumes variation to be accidental is, of itself, sufficient to make us expect some residual phenomenon not accounted for by the law of selection: but Mr Darwin's argument does not allow the existence of such a residuum.

2. Mr. Mivart, following the *North British Review*, has shown the great advantage in the struggle for existence which the numerical superiority in individuals of the original form over the new variation may give to the original form. I want to call your attention to the fact, that the useful variety has to overcome not only this advantage to its antagonist resulting from numbers, but a quite distinct difficulty, namely, the tendency to reversion; or, that in other words, the force which Mr. Darwin has at his disposal is not the tendency to accumulate a variation, but only the balance of this tendency over the tendency to reversion.

It is no doubt difficult to estimate the extent to which one tendency is controlled by the other; but to gain some notion of it, let me assume the existence of three forms, with a male and female of each, viz.,

male	female
a	a' the original form
b	b' the first variety
c	c' the second variety

Let me further indicate the existence of an accumulated variation by the double letter bb, or cc. Let me then assume an equal chance of sexual intercourse between each of the six individuals and of progeny from such intercourse, and we shall see what are the chances of the origin of an accumulated form. For the union of

a	and	a'	produces	a
a	,,	b'	,,	a
a	,,	c'	,,	a
b	,,	a'	,,	a
b	,,	b'	,,	bb
b	,,	c'	,,	a
c	,,	a'	,,	a
c	,,	b'	,,	a
c	,,	c'	,,	cc

So that in the first generation the chance against any given accumulated variation being produced are 8 to 1, and the chances against any accumulated variety at all being produced are 7 to 1; and these odds will go on increasing, because the next generation will retain the tendency to revert to the original simple parent form a, not to the original forms of the last preceding generation only. If I assume that each marriage in the first generation produced two males and two females, the result will be, that in the second generation the chance against any given accumulated variety being produced will be 320 to 4 or 160 to 2; and the chance against any accumulated variety at all being produced will be 316 to 8 or 79 to 2.

These chances express the force of the tendency against which Natural Selection has to operate, assuming that the numbers of each variation at starting were equal to those of the original form; but this is, on the assumption that variation is accidental, infinitely removed from the truth; and if the difference between the numbers of the original form and the variation be introduced into the case, the odds are indefinitely increased against the accumulation of a casual variety. None of these observations goes to show that Natural Selection does not exist (I have

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no doubt that it does), but they show that it has a most uphill game to play, and one in which it is improbable for it to win without help from some other principle.

It may be objected to what I have alleged that the result of the marriages between the individuals of the original form *a* and the varieties *b* and *c*, will not be of the pure original form, but forms half way between that original form and the variety. This will often be the case, but then the improper form of *a* will have in future generations an increasing tendency to revert to the pure original.

3. Mr. Mivart has adduced the co-existence of closely similar structures of diverse origin as evidence that there is a law of Variability over and above the principle of Natural Selection.

The argument appears to me worthy of great consideration. If variability be a force operating in every direction equally, it is very improbable that the course of any two variations should run parellel; if on the other hand the tendency to variation operates along particular lines, then it is likely that the lines which represent the course of separate varieties should run parallel. How do the facts stand? The placental and marsupial quadrupeds form two distinct groups which branched off from one another in very remote times, and yet the two groups have developed into classes and sub-groups so correspondent that their courses may best be described as two parallel zigzag lines. Such a fact almost necessarily involves the conception of one and the same force controlling each group, so as to make it pass along its respective course, just as the likeness of two oak trees implies a like inherent force in each acorn.

But this is not the only instance which Mr. Mivart adduces; he produces the fact that there are two parallel bridges which span the space between birds and reptiles; and I desire to add this further fact not noticed by Mr. Mivart, that there are in like manner two parallel bridges between fishes and reptiles. As is well known, there still exists a small class of animals, half fish and half reptile, often known as the Amphibia. They possess lungs like reptiles, but like fish they have gills and the consequent modification of the hyoidal arch, and their fore limbs have what may be called a degraded and fishlike form. The Archegosauri are extinct creatures of the carboniferous strata—like the existing Amphibia, half reptiles, half fish; like them they carried both gills and lungs, and like them they had the fore limbs in a degraded condition. So far there is nothing to the point; but to the foregoing must be added that the existing Amphibians are a bridge between reptiles and the hard-boned fishes, which are the predominant modern class; whereas the Archegosauri were a bridge between reptiles and the ganoid fishes, to which class they were related by the character of their teeth, the imperfect ossification of the internal skeleton, and the excess of ossification in the external skeleton (Owen's Palæontology, p. 193, *et seq.*). This observation will remain true, even if it should be shown, as some naturalists expect, that the Archegosauri were a tadpole form of a more perfect reptile; for it can hardly be doubted that the immature form expresses the history of the perfect one, and shows by its likeness to the ganoid fishes the original relation to that class.

It has often appeared to me, that there are striking parallelisms in the three great groups of the Quadrumana. For instance, the vocal organs of the Hylobates or Gibbons of the Indian Archipelago present a close relationship to those of the Mycetes or Howlers of the South American forests. The nocturnal and insectivorous habits of some of the American genera recall the like habits of most of the Lemurs: and again, the Baboons are related to the Anthropoid Apes in a way which suggests to the mind that the similarity of their forms is greater than the nearness of their kinship.

There is no doubt that similar parallelisms may be observed in very many groups; and I think that many of those phenomena of likeness which Mr. Darwin would attribute to atavism may as well be explained by the retention in each group of the force which was inherent in the original family from which both groups have proceeded.

This sort of parallelism is well illustrated by a very common accident: a tire gets loose from a wheel; but both it and the wheel pursue nearly parallel courses, except so far as they are operated upon by different external forces.

EDWARD FRY

Protective Resemblances

HAVING read the various papers by Messrs. Bennett, Murray, Wallace, &c., in NATURE, and thinking that my observations made on plants and animals in various parts of Southern Africa may be of some interest to your readers, I am rather hastily putting together a few facts, which it had been my intention to have worked up into an illustrated paper, but which may perhaps prove of most interest at present, while the discussion is still warm.

I must own that I prefer the use of the general term "protective resemblance" to the special term "mimicry," as the latter seems to imply a certain amount of *intelligent volition*, which in the instances cited by Messrs. Wallace, Bates, and Trimen, I believe does not apply, whereas there are, I believe, cases where instinct does come into play, not acting physiologically as Mr. Bennett would seem to assert, but in the construction of disguises.

Mr. Bennett's argument appears to me as fully adverse to his own theory as to Mr. Bates's, and if Mr. Murray's theory is correct then it ought equally to apply to animals so widely different as ants and spiders.

Protective resemblances appear to me to be capable of being roughly classed as general and special, though both run into each other. Of general resemblances there are so many that I hardly think it worth while to enumerate instances, I shall therefore confine myself to some examples of special protective resemblances which I have noted.

1. As to plants I believe protective or useful resemblances are far commoner than some writers seem to think.

That excellent observer Dr. Burchell, in his "Travels," vol. i., p. 10, remarks:—"On picking up from the stony ground what was supposed a curiously shaped pebble, it proved to be a plant, and an additional new species to the tribe of Mesembryanthemum, but in colour and appearance bore the closest resemblance to the stones between which it was growing. On the same ground was found a species of the Gryllus tribe amongst the stones, and so exactly like them in colour and *even in shape* that it could never have been discovered had it not been observed just at a moment when in motion, and as if more completely to elude notice it seldom stirred, and even then but slowly.

"The intention of nature in these instances seems to have been the same as when she gave to the chameleon the power of accommodating its colour in a certain degree to that of the object nearest it in order to compensate for the deficiency of its locomotive powers. By their form and colour, this insect may pass unobserved by those birds, which otherwise would soon extirpate a species so little able to elude its pursuers, and this juicy little Mesembryanthenum may generally escape the notice of cattle and wild animals."

I may here remark that a great number of Karoo plants have tuberous roots of similar form and colour, and it is especially curious to notice that, amongst the Asclepiadeæ, many species, such as *Raphionueme*, which are found in the grassy country, have their tubers hidden beneath the soil, whilst others, which occur in the stony Karoo, such as, *Brachystelma filiforme*, have them above the soil, and so perfectly do they resemble the stones amongst which they are found, that, when not in leaf, it is almost impossible to distinguish them.

Of imitating plants I may mention *Ajuga ophrydis*, the only species of the genus in South Africa, which bears a striking resemblance to an orchid, as also does *Impatiens capensis*, another solitary species. I mention these especially because they are very striking, although I am not aware that they are in any way specially useful, noting, however, that the latter plant is much frequented by insects, often by similar species to those which frequent *Angrœcum* and *Mystacidium*, plants affecting similar localities.

I know of many similar resemblances between plants belonging to most different orders, but cannot say how far they are merely fortuitous, and I am anxious to avoid Mr. Bennett's reproach of being "ultra-Darwinian."

With respect to the orthopterous insect mentioned by Burchell, that excellent observer, Mrs. Barber of Highlands near Grahamstown, communicated a most interesting paper to the Albany Natural History Society on a "Stone Grasshopper," which varies according to the nature of the soil of the district it inhabits; and I have in my collection numerous species of Locustidæ collected in the Karoo, whose successful imitation to the soil is most remarkable. I may remark here, that many most singular species of insects lose half their interest when penned down in a collector's cabinet.

As mimicry amongst mammals and reptiles is noted as rare, I may mention instances of what appear to me to be cases.

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